pons varolii, or in the neighbouring parts, appears to have produced paralysis in both sides of the body. 7th. Cases in which an alteration in one side of the pons varolii, or of the neighbouring parts, has produced paralysis in the same side of the body. 8th. Cases and experiments which appear to prove that there are, in various parts of the encephalon and in the spinal cord, motor nerve-fibres which are not voluntary motor. 9th. Anatomico-pathological dissections—as those of Dr. L. Türck, of Vienna—which appear to prove that there are nerve-fibres coming from the spinal cord which decussate in parts above the medulla oblongata.

After a careful examination of these several class of facts, Dr. S. presents the following as his conclusions in regard to them:—

"1st. That reflex movements alone, and not sensations and volitions, exist in monsters deprived of a great part of their cerebro-spinal axis.

"2d. That, when the spinal cord, the medulla oblongata, or the pons varolii are altered, even considerably, sensibility and volition may continue to exist, because there are still communications by nerve-fibres through the altered parts, between the nerves of the trunk and limbs and the parts of the encephalon, in front of the pons.

"3d. That, if the reasons given by many physiologists to prove that the pons Varolii is the seat of the centre for volition, and for perception of sensitive impressions, were true, we should have to admit that the medulla oblongata is the centre—or, at least, a part of the centre—for these faculties, because the same reasons appear to prove so for this organ as for the pons.

"4th. That very likely these faculties have not their centre, at least their principal centre, in the pons Varolii, and still less in the medulla oblongata.

"5th. That it seems that, in some men, the sensitive and voluntary motor nerve-fibres do not decussate at their usual place, and that, in consequence, some rare cases may exist in which an alteration in one side of the pons Varolii, or of the medulla oblongata, will produce paralysis of the same side of the body.

"6th. That there appears to be, in many places of the encephalon, nerve-fibres which are not voluntary motor, and which, nevertheless, go to muscles either on the same side of the body as that of the encephalon from which they originate, or on the opposite side, and that these muscular nerve-fibres are able to produce convulsions when they are irritated by an injury or an alteration in the encephalon, so that convulsions may take place either on the paralyzed side or on the other.

"7th. The results of the researches of Dr. Ludwig Türck cannot, in the actual state of science, prove against or in favour of any doctrine relative to the place of decussation of sensitive and voluntary nerve-fibres."

As this most interesting work of Dr. Séquard may not fall into the hands of a number of our subscribers, we have endeavoured to give them a pretty full account of the results to which the author has been led by his researches on the physiology and pathology of the spinal cord and other portions of the nervous centres. Though in opposition to generally received opinions, their importance, and the close and logical manner in which the investigations upon which they are based have been conducted by Dr. S., press strongly upon our attention his conclusions in respect to the place of decussation of the sensitive and motor nerve-fibres.

D. F. C.

Art. XXIV.—*Observations on Yellow Fever, and its Relations to Quarantine and other Hygienic Measures.* By S. L. Grier, M. D. 8vo. pp. 41. New Orleans, 1854.

In relation to many questions, medical opinion would appear to move not forward, but in a circle, and hence we have the constant recurrence of doctrines which were once current for a period, and then repudiated for others of perhaps a precisely opposite character. In nothing is this more strikingly shown than in the views entertained in regard to the etiology of yellow fever. The doctrine of its invariable importation into Europe and into this country by fomites or patients sick of the disease; of its absolute contagion under all circumstances, or its propagation by fomites, or a specific morbid emanation from the bodies of the sick, under certain conditions of impurity and elevation of temperature in the atmosphere of particular localities, which very generally prevailed during the latter portion of the eighteenth century, and which most of us supposed now almost entirely abandoned by the leading members of the profession both at home and abroad. When we had believed that the opposite doctrine of the local origin of the disease, and its non-contagious character, was become so clearly established by an overwhelming mass of well-authenticated facts as to be very generally received, we find efforts being made in more than one direction to discredit the latter and revive the former.

To effect this, and in the train of the old hypothesis to subject us to all the expense, discomfort, and folly of a system of rigid quarantine, is the object of the publication before us.

Dr. Grier maintains that the yellow fever is not of American origin; that its source can be traced to the eastern continent, in the same manner that we trace epidemic cholera to its nativity in remote India. He admits, nevertheless, that it is now *naturalized* or endemic in certain parts of inter-tropical America. The exact limits of this yellow fever zone, north and south, have, however, not yet been determined.

Dr. G. believes it possible for the yellow fever to prevail in New Orleans either *sporadically*, that is, as "an occasional disease, occurring without any general or prevailing cause;" or *endemically*, that is, as a disease peculiar to the inhabitants of the place—a disease native to the locality; or *epidemically*, that is, as a disease which, although prevalent among the inhabitants of a city or country, is not necessarily native in that locality, and may be extended to other places either neighbouring or remote.

Dr. G. would seem to make a distinction, not in the cause or the more general diffusion of the cause, between yellow fever as an endemic and yellow fever as an epidemic, but in the very nature of the disease itself. Thus, while he admits that by naturalization it has become endemic in—in other words, a disease of New Orleans and other American cities, yet he asserts that

"Yellow fever, *in its epidemic form*, does not originate in the cities of the United States where it has hitherto prevailed, but has *always* been introduced either in the hold of vessels coming from infected ports, in goods, or by subjects of the disease.

"The epidemic virus thus conveyed to our shores is not extended and diffused through the medium of the atmosphere, except to a very limited extent, differing in this respect from a class of epidemics of which *influenza* may be offered as an example, but that it is more allied in its mode of propagation and manner of progression to another class of epidemics which are known to be contagious in their character, of which class *scarlet fever* may be cited as an instance."

Dr. G. includes yellow fever in that class of contagious diseases "which are transmitted from the sick to the well through the medium of the atmosphere immediately surrounding the patient, but only under certain conditions, by virtue of which the atmosphere is rendered more liable to be the conductor of the specific virus."

These "certain conditions" are high ranges of temperature, and that form of malaria produced by undue moisture, marsh effluvia, filth, defective ventilation, and over-crowded population. None or all of those conditions, however, are, according to Dr. G., capable of producing the yellow fever. The combination of a foreign or external cause—the virus of the disease—with these local conditions is, he maintains, essential to its production and propagation. Hence he maintains that yellow fever, *as an epidemic*, is an imported disease—"the *foreign element* which is prerequisite to its production being brought to our shores by the ships of commerce, but is incapable of engendering the disease in this form unless the domestic conditions necessary to the propagation of an epidemic influence are present also."

Now, unless Dr. G. supposes that there are two distinct forms of yellow fever, the endemic and the epidemic, he has admitted too much for the safety of his unqualified dictum that epidemic yellow fever has always been introduced from without into every city of the United States where it has hitherto prevailed. In certain of these cities he informs us that the disease may occur either sporadically or as an endemic; what, then, according to his own showing, is to prevent the disease from prevailing epidemically whenever the malaria, the heat, and the other local elements necessary to the propagation of an epidemic influence, are present also?

We shall not adduce, in refutation of the unqualified dictum of Dr. G. of the invariable importation of yellow fever into the United States, the well-authenticated instances of the occurrence of the disease in our midst, in years when no vessel had arrived, for many months previously, from any port at which the disease prevailed, nor the numerous instances in which the disease has broken out on board of vessels in the midst of the ocean, thousands of miles from land—vessels that had sailed from a healthy port, with a healthy crew; nor still, the fact that neither the most rigid seclusion nor the most rigorous quarantine has always afforded protection against the disease, while, in some instances, it would appear to have increased the certainty of an attack: we shall not adduce these facts, because Dr. G. may be inclined to answer them by asserting that in neither of the circumstances referred to was the yellow fever epidemic, and reminding us that the views he has advanced have relation solely to the disease in its epidemic form.

Dr. G. has presented a show of evidence in support of his opinions, of which it is not our intention, on the present occasion, to enter into an examination. All of it having a direct bearing upon the leading points discussed in the essay before us, has been carefully weighed by those fully competent to the task, and shown to be altogether inadequate to sustain either the importation of yellow fever or its propagation by contagion.

By the General Board of Health of Great Britain, all the facts connected with the origin and propagation of yellow fever have been carefully examined and collated. The conclusion at which the Board have arrived, and which they announced to Parliament in their second report, is—

"That there is no evidence to prove that yellow fever has ever been imported.

"That, consequently, the means of protection from the disease are not quarantine restrictions and sanitary cordons, but sanitary works and operations, having for their object the removal and prevention of the several localizing conditions, and when such permanent works are impracticable, the temporary removal, as far as may be possible, of the population from the infected localities."

When we reflect that those conclusions are drawn not from a few isolated facts, but from a mass of testimony extending through the history of the yellow fever from 1793 to 1851, and derived from the most impartial observers who had ample opportunities of investigating every point connected with the etiology of the disease, we cannot but acknowledge their force; more especially, when such men as Edwin Chadwick and T. Southwood Smith, by whom the report alluded to was drawn up, assure us—

"That from the most careful examination which we have been able to make of the mass of evidence submitted to us, from which the foregoing conclusions have been deduced, we have not found a *single fact* or *observation* clearly ascertained, and authentically recorded, opposed to the general tenor of such evidence. We have met with no exceptional cases. We have, indeed, found the opinions of some authorities, for whom we entertain great respect, not in accordance on some points; but these have reference, for the most part, to matters of a purely professional and scientific nature. On the great practical question, whether, whatever may be the nature and mode of propagation of yellow fever, quarantine and sanitary cordons can afford any real protection against its introduction and spread, we believe there is now a very general unanimity of opinion that, in accordance with the evidence we have submitted, they cannot. We believe there is the like general agreement in this further practical conclusion, that the substitution of sanitary or hygienic measures for quarantine isolation and restriction, would afford more certain and effectual protection." D. F. C.